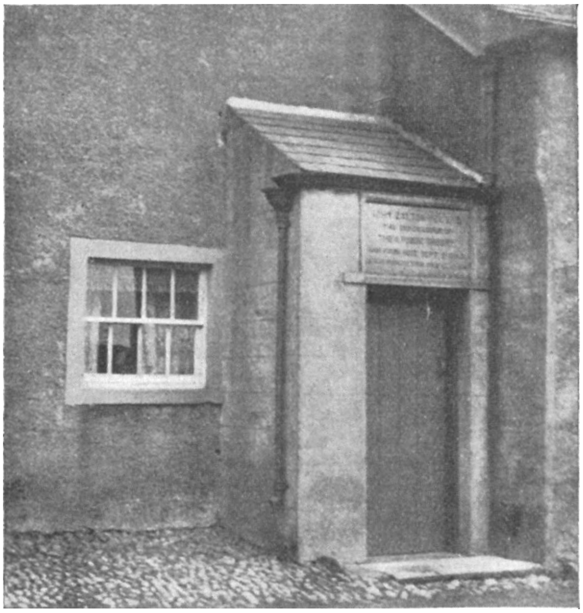
was in the chair. The address was presented by Mr. Norman Smith, a former student under Prof. van 't Hoff. The professor, who was enthusiastically received, said the question was often asked, nowadays, whether the atomic theory had not outlived its utility. His reply was that, in dealing with natural phenomena, with states of unstable equilibrium, the atomic theory was indispensable for essential explanations. He had come to regard the conception of the carbon atom as the centre of a tetrahedron as childish, but it contained the germ of a profound truth, the solution of which must be left to the future. He suggested that valency was due to an equilibrium. The four mutually repellent "electric atoms" of Helmholtz were kept in equilibrium by their attraction for the carbon atom at the centre.

Later in the morning Earl Spencer, Chancellor of the Victoria University, conferred the honorary degree of Doctor of Science on Prof. Clarke and Prof. van 't Hoff, who were presented by Prof. Dixon. After the conclusion of the ceremony Prof. van 't Hoff laid the first stone of the proposed extension of the Owens College Chemical Laboratories, and was presented, as a memento of the occasion, with a silver trowel by the College Chemical Society. The celebrations were concluded by a soirée held at the Owens College on Thursday night, when Dr. Harden gave an interesting account of John Dalton, and many Dalton relics were exhibited by the Manchester Literary and Philosophical Society, Prof. H. B. Dixon, Mr. Theodore Neild, Mr. G. W. Graham and Mr. G. S. Woolley. E. C. E.



Memorial Tablet over door of house in which John Dalton was born. From a photograph supplied by Mr. A. Humphreys. The inscription on the tablet reads:—"John Dalton, D.C.L., LL.D., the Discoverer of The Atomic Theory, was born here Sept. 6, 1766. Died at Manchester July 27, 1844."

THE ATOMIC THEORY AND THE DEVELOPMENT OF MODERN CHEMISTRY.

MANCHESTER celebrated last week, just a little prematurely, the centenary of John Dalton's atomic theory. It was on September 6, 1803, that he drew up in his notebook his first table of weights of the "ultimate atoms" of hydrogen (which he took as his unit), oxygen, "azot," carbon, sulphur, and of water, ammonia, nitrous gas, nitrous oxide, and other binary compounds of these elements. With regard to the genesis of the theory in his own mind much doubt has prevailed until recently. Dalton himself told Thomas Thomson in 1804 that he had been led to the theory from his work on marsh gas and olefiant gas. He told W. C. Henry in 1824 that his speculations were suggested by the work of Richter. And yet, oddly enough, as Sir Henry Roscoe and Dr. Harden have shown in their "New View of Dalton's Atomic Theory" the evidence is dead against the accuracy of these plausible statements. Dalton's own notebook shows that his atomic theory preceded his work on marsh gas, and his notes for a lecture delivered in 1810 give a history of his ideas which agrees with all the facts.[1]

It was from Newton that Dalton derived his belief in the atomic hypothesis. And we can trace the "solid massy, hard, impenetrable, moveable particles" of Newton, through his friend Boyle, through Gassendi, and through Bacon (who considered Democritus to be the greatest of Greek philosophers) back to Epicurus and to the originators of the atomic theory, Democritus and Leucippus. Dalton's theory of atoms is historically the Greek theory of atoms. But with a difference.

Boyle, who was a far more thoroughgoing atomist than is generally supposed, really rejects the hypothesis of different elements which he himself originated, considering that differences of atomic structure and arrangement of a single form of matter would account for all chemical transformations.[2]

But Boyle's own definition of an element, as a substance which could not be decomposed, proved far more fruitful than his atomic beliefs, and the work of his successors—of Marggraf, of Black and Cavendish, of Scheele and Bergman, of Priestley and Lavoisier—had gradually established in the minds of chemists the idea, rejected by Boyle, that there existed a series of elements not convertible into one another. It was to that series of elements, unknown to the ancients, that Dalton applied the atomic hypothesis. He came to the conclusion that the atoms were not of all kinds of shapes and forms, as had been previously supposed, but that the atoms of the same element were all identical in weight, while the atoms of different elements were different in weight. It was an idea that might conceivably have occurred to some chemist fifty years earlier. But, in spite of Black's work, the phlogiston theory had led chemists before Lavoisier to lay small stress on the notion of weight. Dalton could hardly have come much earlier than he did. The first announcement of his theory was made in a paper read in October, 1803, at a meeting of the Manchester Literary and Philosophical Society, in the house of which he had his laboratory; the paper was not published until 1805. Dalton's views were not fully placed before the world until the publication of the first volume of his "New Systems of Chemical Philosophy," in the years 1808–1810.[3]

Meanwhile Dalton had been carrying out researches which confirmed his view, and, together with certain assumptions, led to the most important of generalisations. Dalton himself never disengaged the facts from the theoretical language in which he clothed them. But we may say, broadly speaking, that Dalton's atomic theory led to the establishment of three fundamental laws of chemistry, the law of definite proportions,[1] the law of multiple proportions (which really includes the law of definite proportion) and the law of equivalents. The fact that elements unite in more than one ratio by weight obviously made further assumptions necessary, over and above the atomic hypothesis, before any table could be drawn up of relative atomic weights. Dalton seemed to have felt no hesitation in making the assumptions that seemed to him convenient ("New System," part i. p. 214). But Wollaston, while giving Dalton's theory his powerful support, showed, in 1814, that Dalton's assumptions were arbitrary, and Wollaston's term "equivalent," which was regarded as implying no hypothesis, soon became a serious rival to the term "atomic weight." Davy, to whom (with Henry) Dalton had dedicated part ii. of the "New System" in 1810, gave Dalton's views a reception more than cool.[2]

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[1] Save for an obvious clerical error of 1805 for 1803.

[2] "I see not, why we must needs believe, that there are any primogeneal and simple bodies, of which, as of pre-existent elements, nature is obliged to compound all others. Nor do I see why we may not conceive, that she may produce the bodies accounted mixt out of one another by variously altering and contriving their minute parts, without resolving the matter into any such simple or homogeneous substances, as are pretended" ("The Sceptical Chymist," part vi., folio edition, vol. i., p. 369). See also p. 366, a reference to an experiment by which Boyle thought he had "destroyed refined gold and brought it into a metalline body of another colour and nature"; and p. 367, an earlier announcement of the view just quoted.

[3] The first part of this volume appeared in 1808, the second in 1810. The first part of the second volume only appeared in 1827. The work was not completed.

Among the great chemists of the day, it was to Berzelius, who had already been trying to extend the quantitative work of Richter, that Dalton's views appealed most. But Berzelius, less imaginative, but more critical a thinker and more accurate a worker, than Dalton, saw that much remained to be done before the theory could be placed on a satisfactory basis. "I think," he writes to Dalton, "that we must let experiment mature the theory." Berzelius's admirable "Essai sur les Proportions chimiques" of 1819[3] gives the first critical account of the atomic theory, while the experiments recorded therein may be regarded as having first placed the laws of multiple proportions and of equivalents on a sufficiently wide basis to be regarded as generally valid.

Nevertheless, the conviction that chemistry could do quite well without the conception of atoms, and that the notion of "equivalents" was sufficient, grew steadily; between 1840 and 1850 Leopold Gmelin's system of equivalents came to be accepted almost universally.[4] It was the growth of organic chemistry and the confusions in organic chemistry which the "equivalent" conception was powerless to remove that restored the notion of the atom. From 1842 onwards Laurent and Gerhardt, those two Ishmaels of their day, fought indefatigably for the establishment of some consistent theory of organic compounds; and they reached consistency only by reviving the simple molecular hypothesis of Avogadro and Ampère.[5] This hypothesis gave them at once an experimental method for the determination of the relative molecular weights of all volatile compounds; and it gave them simultaneously a method for determining maximum values for the atomic weights of the elements therein contained, for obviously each molecule must contain at least one atom. But neither they, nor Cannizzaro later, were able to give any simple rule applicable in all cases to the determination of atomic weights. The atomic weight of carbon on which the reform of Laurent and Gerhardt pivoted was an exception to the rule of Dulong and Petit on which Cannizzaro, with general approval, has laid so much stress. But a hypothesis may be useful without being perfect. The atomic hypothesis in the hands of Wurtz, Hofmann, Williamson, Frankland, Kekulé, and Baeyer, and with the most brilliant and essential but involuntary help of Berthelot and of Kolbe, was the instrument which served to build up modern organic chemistry. It gave chemists an unforeseen mastery over the elements; the synthesis not only of the natural organic compounds, but of an infinity of new ones seemed to be brought within their reach. In this development Manchester had again played a part of first-rate importance. Frankland's theory of valency was based on his researches on the organometallic bodies carried out in the Owens College, where he was professor, and published in 1852. The exact *rôle* of Frankland's work on valency (neglected at first by most chemists) was this: it forced his friend and fellow worker, Kolbe, to abandon the Berzelius copula theory, and led him to build up "constitutional" formulæ for the chief alkyl compounds so near our own that he was enabled to predict from them the existence of secondary and tertiary alcohols. The formulæ of Kolbe, with the atomic weights of Gerhardt, again led inevitably to the great theories of Kekulé on the tetravalency of carbon and the linking of the atoms, which are now regarded as fundamental in organic chemistry.

In 1875, new horizons were brought into view. Wollaston predicted of Dalton's atoms in 1808 that "the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension." Le Bel and van 't Hoff, by their work on the "asymmetric" carbon atom, created a new "chemistry in space," of which one of the most striking results has been the beautiful synthesis of the sugars, by Emil Fischer and his fellow workers. Prof. Pope has recently extended these new ideas to inorganic chemistry with brilliant results.

But such exceptional results as those of Prof. Pope bring sharply into view the fact that the direct service of the atomic theory to inorganic chemistry has been relatively small. What, for instance, has the theory of valency to tell us about such a series of compounds as the tungsten chlorides discovered by Roscoe? But if the atomic theory has helped us comparatively little in determining the constitution of inorganic compounds,[1] it has contributed to our discovery of new inorganic elements. The attribution of certain numbers, equivalents or atomic weights, to the elements led naturally to speculation on mathematical relationships between them. Many of these speculations, like the original one of Prout in 1815, and that of Dr. Henry Wilde, of Manchester, more recently, were suggested by the fascinating question of the fundamental unity of all matter. Are the elements really compounds of one original matter—the *protyle* of the Greeks revived by Prout and by Sir W. Crookes? If so the atomic weights must have some common measure. On the accurate determination of atomic weights, made largely to settle this question, infinite pains have been spent by Stas, Marignac, Richards, and many others. On the criticism and accurate calculation of results from these experimental determinations infinite pains have again been spent, by Meyer and Seubert, and above all by Prof. F. W. Clarke, who delivered the Wilde lecture of the Manchester Literary and Philosophical Society at the centenary celebrations last week.

But though certain numerical relations seem striking, chemists are certainly as a body not inclined to acknowledge the existence of any exact formula expressing as a mathematical series the series of the atomic weights.

More immediately fruitful of results have been speculations less fundamentally ambitious. The schemes of Lothar Meyer and Mendeleeff, according to which the elements, when arranged in the order of their atomic weights, take their place on a kind of chessboard, elements resembling one another being in the same row, have led to the prediction of the existence of new elements; and even unpredicted new elements, such as the remarkable series discovered by Lord Rayleigh and by Sir William Ramsay, have had a fairly comfortable place found for them by extending the chessboard on ground to which it had some legitimate claims.

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[1] The present writer has briefly discussed the history of this law, in Nature, vol. l. 1894, p. 149.

[2] In two unappreciative lines in a footnote to the "Elements of Chemical Philosophy," published in 1812 (see p. 78 of the edition of 1860).

[3] The Swedish edition appeared earlier.

[4] Gmelin himself in his "Handbook of Chemistry" inclined to the atomic theory. English edition, translated by H. Watts, vol. i. p. 42.

[5] "Equal volumes of all gases under the same conditions of temperature and pressure contain equal numbers of molecules."

[1] The researches of Divers and of Raschig on certain sulphur and nitrogen compounds may be regarded as examples of what may be done in this direction.

Inorganic chemistry has developed recently very largely on the physical side. In much of the work, notably in the applications of thermodynamics (and especially of the researches of Willard Gibbs, whose death we lament), the atomic theory plays no part, or but a small one. In the great studies on solutions, however, originated by van 't Hoff, Arrhenius, and Ostwald, the fruitful *ion* theory formulated by these chemists can hardly be regarded as independent of the atomic theory. And yet, in his last book on inorganic chemistry, Prof. Ostwald employs "the forms of the atomic hypothesis as sparingly as ever the present use of language will permit."[1]

In what has preceded, the atomic theory has been regarded from the point of view of utility. Of its utility to chemistry there can be no doubt. It helps us to describe complicated phenomena briefly. The atomic formula $CH_3.COOH$ reminds organic chemists at a glance of a very large number of properties of acetic acid. But, many will ask, is this atomic theory something more than useful? Is it really true?

The subject has been much discussed of late both by men of science and philosophers.[2] One school regards the methods of experimental science as capable of yielding generalisations that are absolutely true, and some of the members of this school do not hesitate to say that the atomic theory is absolutely true. Sir Arthur Rücker concluded his brilliant address to the British Association in 1901 by declaring that "we have a right to insist—at all events till an equally intelligible rival hypothesis is produced—that the main structure of our (*i.e.* the atomic) theory is true; that atoms are not merely helps to puzzled mathematicians, but physical realities." Even in this most positive assertion of Sir Arthur Rücker with regard to the existence of atoms there remains a shade of doubt. Lord Kelvin, in a subsequent speech, showed that in his mind, at any rate, there was none.

There is, however, another school, the origins of which go back far, but which is identified chiefly with Kirchhoff (the discoverer with Bunsen of spectrum analysis), and with his disciples Mach and Ostwald in Germany, and Karl Pearson in England. According to this school, the discovery of "causes" and of ultimate truths is not the business of experimental science. The object of science, according to Kirchhoff, is to describe natural phenomena in the simplest way possible. If a theory like the atomic theory helps us to describe observed phenomena more simply and to discover new ones, let us use it by all means. But (they would say) since the existence of atoms cannot be verified directly,[3] it is really useless for scientific purposes to discuss whether the theory is true or not. Obviously, science here abandons those claims to finality which have been insisted on so strongly by the older and more orthodox school, for our simple descriptions are liable at any moment to be replaced by descriptions still more comprehensive and still more simple. It would be hard indeed to prove that any given theory has attained a maximum of simplicity in summarising the facts with which it deals.

Kirchhoff's self-denying ordinance on the part of science leaves, no doubt, a wider field open to the metaphysicians. But *qui trop embrasse mal étreint;* and the limitations of scientific claims which he advocates may well strengthen science in her own proper borders.

The atomic theory has had a long and venerable history; the "solid, impenetrable" particles of Newton were originated by the Ionian philosophers in the fifth century B.C. A hundred years ago the genius of Dalton gave the theory a fresh and still unfinished career of usefulness, and whether we consider it in the light of a truth that cannot ever disappear from science, or rather as an engine serving to fashion and unite our ideas, possibly to be replaced later by an intellectual mechanism still more efficient, our debt to Dalton remains one of the greatest that the world owes to its great men.

P. J. HARTOG.

NOTES.

A SPECIAL meeting of the Physical Society will be held on Friday, June 5, at 5 p.m., at University College, when Prof. Rutherford, of Montreal, will read a paper on radioactive processes. A discussion will follow, in which it is hoped several prominent physicists will participate.

IN reply to a question asked in the House of Commons on Tuesday, Mr. Balfour stated that the Government would contribute to the funds required to send the relief ship *Morning* to the Antarctic at the end of this year, to ensure the safety of the officers and men of the *Discovery*, now ice-bound in Antarctic seas.

THE ninth quinquennial conference of the States adhering to the International Telegraph Convention was opened on Tuesday by the Postmaster-General, Mr. Austen Chamberlain, M.P. The business of the conference will go on from day to day until the end of June. Mr. J. C. Lamb, C.B., C.M.G., the principal delegate of Great Britain, was chosen president of the conference, and Mr. John Ardron and Mr. P. Benton vice-presidents.

M. HENRI BECQUEREL, Paris, and Prof. A. Righi, Bologna, were elected honorary fellows of the Physical Society of London at the last general meeting.

THE *Daily Mail* announces that Mr. Andrew Carnegie has offered to subscribe 10,000*l.* towards the erection of an experimental tank for testing ship models, as a memorial to James Watt.

MR. ANDREW CARNEGIE has offered to give 200,000*l.* for a building for the American engineering societies. It is, says *Science*, to be situated in New York City, and will provide an auditorium, a library and headquarters for five engineering societies.

THE death is announced of Prof. C. A. Bjerknes, professor of pure mathematics at the University of Christiania, at the age of seventy-eight, and of Dr. G. C. Dibbits, formerly professor of chemistry at Utrecht, at the age of sixty-four.

THE death is announced of M. Félix Worms de Romilly, a former president of the French Physical Society, who served for many years on the council, and who in addition contributed liberally towards the cost of certain publications undertaken by the Society.

THE *Bulletin de la Classe des Sciences* (Brussels) announces the death, at the age of seventy-six, of M. Charles de la Vallée Poussin, professor of mineralogy and geology of the University of Louvain, and author of important geological papers published in the *Bulletin* itself

[1] "Principles of Inorganic Chemistry." Translated by A. Findlay, 1902, p. 146. (Macmillan and Co., Ltd.)

[2] See Prof. James Ward's "Naturalism and Agnosticism," 2 vols., 1899.

[3] "No physicist or chemist can produce a single atom separated from all its fellows and show that it possesses the elementary properties he assigns to it" (Sir A. Rücker, *loc. cit.*).